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Introduction

The Lessons and Challenges of Social Psychology • The Tripod on Which Social Psychology Rests • Predictability and Indeterminacy • The Problem of Effect Size • Overview and Plan of the Book

Undergraduates taking their first course in social psychology generally are in search of an interesting and enjoyable experience, and they rarely are disappointed. They find out many fascinating things about human behavior, some of which validate common sense and some of which contradict it. The inherent interest value of the material, amounting to high-level gossip about people and social situations, usually ensures that the students are satisfied consumers.

The experience of serious graduate students, who, over the course of four or five years, are immersed in the problems and the orientation of the field, is rather different. For them, the experience is an intellectually wrenching one. Their most basic assumptions about the nature and the causes of human behavior, and about the very predictability of the social world, are challenged. At the end of the process, their views of human behavior and society will differ profoundly from the views held by most other people in their culture. Some of their new insights and beliefs will be held rather tentatively and applied inconsistently to the social events that unfold around them. Others will be held with great conviction, and will be applied confidently. But ironically, even the new insights that they are most confident about will tend to have the effect of making them less certain than their peers about predicting social behavior and making inferences about particular individuals or groups. Social psychology rivals philosophy in its ability to teach people that they do not truly understand the nature of the world. This book is about that hard-won ignorance and what it tells us about the human condition.

THE LESSONS AND CHALLENGES OF SOCIAL PSYCHOLOGY

As graduate students at Columbia University in the 1960s, working primarily with Stanley Schachter, we underwent the experience typical of students exposed to the experimental tradition in social psychology. That is, many of our most fundamental beliefs about human behavior, beliefs that we shared with most other people in our culture and that had remained intact or even been strengthened by our undergraduate courses in the humanities, were abruptly challenged in ways that have shaped our subsequent careers. An introduction to these challenges, which we offer below, provides a departure point for our discussion of the contributions of our discipline. Indeed, the remainder of our book represents an attempt to reconcile common sense and common experience with the empirical lessons and challenges that lie at the core of social psychology. In so doing, the book seeks to provide an overview of social psychology's primary scientific and intellectual contributions, one that serves to challenge, reform, and expand common sense.

The Weakness of Individual Differences

Consider the following scenario: While walking briskly to a meeting some distance across a college campus, John comes across a man slumped in a doorway, asking him for help. Will John offer it, or will he continue on his way? Before answering such a question, most people would want to know more about John. Is he someone known to be callous and unfeeling, or is he renowned for his kindness and concern? Is he a stalwart member of the Campus Outreach Organization, or a mainstay of the Conservative Coalition Against Welfare Abuse? In short, what kind of person is John and how has he behaved when his altruism has been tested in the past? Only with such information in hand, most people would agree, could one make a sensible and confident prediction.

In fact, however, nothing one is likely to know or learn about John would be of much use in helping predict John's behavior in the situation we've described. In particular, the type of information about personality that most laypeople would want to have before making a prediction would prove to be of relatively little value. A half century of research has taught us that in this situation, and in most other novel situations, one cannot predict with any accuracy how particular people will respond. At least one cannot do so using information about an individual's personal dispositions or even about that individual's past behavior.

Even scientists who are most concerned with assessing individual differences in personality would concede that our ability to predict how particular people will respond in particular situations is very limited. This "predictability ceiling" is typically reflected in a maximum statistical correlation of .30 between measured individual differences on a given trait dimension and behavior in a novel situation that plausibly tests that dimension. This ceiling, for example, would characterize our ability to predict on the basis of a personality test of honesty how likely different people will be to cheat in a game or on an exam, or to predict on the basis of a test of friendliness or extroversion how much sociability different individuals will show at a particular social gathering. Now a correlation of .30, as we will emphasize later, is by no means trivial. Correlations of this magnitude can be quite important for many prediction purposes. But a correlation of .30 still leaves the great bulk of variance in people's behavior unaccounted for. More importantly, a correlation of this magnitude is a good deal lower than it would have to be to provide the type of predictability that most laypeople anticipate when they make predictions about each other's behavior or make inferences about others' personal attributes. Moreover, the .30 value is an upper limit. For most novel behaviors in most domains, psychologists cannot come close to that. Certainly, as we will see, neither the professional nor the layperson can do that well when obliged to predict behavior in one particular new situation on the basis of actions in one particular prior situation.

Despite such evidence, however, most people staunchly believe that individual differences or traits can be used to predict how people will behave in new situations. Such "dispositionism" is widespread in our culture. What is more, most of us, scientists and laypeople alike, seem to find our dispositionism affirmed by our everyday social experience. The challenge of accounting for this discrepancy between beliefs about everyday experience on the one hand and empirical evidence on the other hand is one of the most important faced by psychologists. We will deal with it at many points in this book.

The Power of Situations

While knowledge about John is of surprisingly little value in predicting whether he will help the person slumped in the doorway, details concerning the specifics of the situation would be invaluable. For example, what was the appearance of the person in the doorway? Was he clearly ill, or might he have been a drunk or, even worse, a nodding dope addict? Did his clothing make him look respectably middle class or decently working class, or did he look like a homeless derelict?

Such considerations are fairly obvious once they are mentioned, and the layperson, upon reflection, will generally concede their importance. But few laypeople would concede, much less anticipate, the rel-

evance of some other, subtler, contextual details that empirical research has shown to be important factors influencing bystander intervention. Darley and Batson (1973) actually confronted people with a version of the situation we've described and found what some of these factors are. Their subjects were students in a religious seminary who were on their way to deliver a practice sermon. If the subjects were in a hurry (because they thought they were late to give a practice sermon), only about 10 percent helped. By contrast, if they were not in a hurry (because they had plenty of time before giving their sermon), about 63 percent of them helped.

Social psychology has by now amassed a vast store of such empirical parables. The tradition here is simple. Pick a generic situation; then identify and manipulate a situational or contextual variable that intuition or past research leads you to believe will make a difference (ideally, a variable whose impact you think most laypeople, or even most of your peers, somehow fail to appreciate), and see what happens. Sometimes, of course, you will be wrong and your manipulation won't "work." But often the situational variable makes quite a bit of difference. Occasionally, in fact, it makes nearly all the difference, and information about traits and individual differences that other people thought all-important proves all but trivial. If so, you have contributed a situationist classic destined to become part of our field's intellectual legacy. Such empirical parables are important because they illustrate the degree to which ordinary men and women are apt to be mistaken about the power of the situation—the power of particular situational features, and the power of situations in general.

People's inflated belief in the importance of personality traits and dispositions, together with their failure to recognize the importance of situational factors in affecting behavior, has been termed the "fundamental attribution error" (Ross, 1977; Nisbett & Ross, 1980; see also Jones, 1979; Gilbert & Jones, 1986). Together with many other social psychologists, we have directed our attention to documenting this conjoint error and attempting to track down its origins. Every chapter of this book will discuss research relevant to this error. In Chapter 5 we will marshall the evidence showing how widespread the error is and try to explain why it occurs.

The Subtlety of Situations

There is another face to situationism. Not all situational factors prove to be powerful determinants of behavior, not even those that seem intuitively strong to both laypeople and social scientists. Some, in fact, prove to be astonishingly weak.

Nowhere is the weakness of apparently big situational factors more perplexing than in studies of the impact of various real-life events on important social outcomes. For some of these weak effects we can be

grateful. For example, it turns out that in most cases the long-term impact of physical and sexual abuse suffered in childhood is relatively slight (Widom, 1989), as is the long-term effect of teenage pregnancy on a young woman's life outcomes (Furstenberg, Brooks-Gunn, & Morgan, 1987), and even the long-term effect of P.O.W. camp indoctrination (Schein, 1956). Unfortunately, apparently positive events sometimes also prove to be surprisingly weak in their effect. For example, the lives of major lottery winners seem to be influenced far less by their windfalls than most of us would predict, especially when we imagine how much our own lives would be changed by a similar windfall (Brickman, Coates, & Janoff-Bulman, 1978).

A more sobering example of the weakness of apparently large, apparently positive events is to be found in what is perhaps the progenitor of modern social intervention experiments, the Cambridge-Somerville study of delinquency described by Powers and Whitmer (1951) with follow-ups by the McCords (J. McCord, 1978; J. McCord & W. McCord, 1959; W. McCord & J. McCord, 1959). The subjects in this noble experiment (which we discuss at greater length in Chapter 8 on applications of social psychology) were both "delinquency prone" and "average" boys living in a lower socioeconomic status in a mostly Irish and Italian suburb of Boston in the 1940s. Some of the boys were assigned to an extremely ambitious and intensive experimental intervention condition in which, over roughly a 5-year period, they were exposed to a wide variety of social, psychological, and academic supports. Thus counselors provided two home visits per month to work on personal and family problems. Tutoring in academic subjects was made available. Many of the boys received psychiatric or medical help. Contact with Boy Scouts, YMCA, or other community programs was facilitated, and a substantial number of the boys were given the opportunity to attend summer camps. Despite this intensive and apparently favorable intervention, however, the boys in this experimental, or "treatment," condition proved to be no less likely to become delinquent than those in an "untreated" control group. Indeed, follow-ups conducted 30 years after the end of the program suggested that treated subjects may actually have fared slightly worse as adults, for example, in terms of rates for serious adult offenses, than those subjects whose outcomes were merely monitored.

Follow-up research on the nondelinquent boys in the Cambridge-Somerville sample who received no treatment (Long & Vaillant, 1984) showed even more surprising noneffects—in this case, noneffects of apparently important social factors in the boys' family backgrounds. The boys were classified into four different categories depending on the degree of social health or pathology of their home life. At the lowest extreme were families with many serious problems—for example, an alcoholic or abusive father, a schizophrenic mother, a dependence on many social agencies for financial support, and so forth. At the opposite

extreme were families that seemed for the most part to be models of the working poor—fathers were employed, mothers were serving as homemakers, there was no obvious pathology and no dependence on social agencies. The life outcomes of boys in these different categories were then examined in a follow-up study 40 years later. On indicator after indicator—for example, income, mental health, prison incarcerations, suicides, and the like—the status of the subjects' home situation as children made little if any difference.

What do we learn from these spectacular noneffects? Certainly not that situational factors are unimportant in the world outside the social psychology laboratory. As we will see beginning in Chapter 2, many real-world effects turn out to be huge—from the dramatic personal changes wrought by immersing conservative young women in highly liberal surroundings (Newcomb, 1943), to the pronounced effect of competition on group conflict (Sherif, Harvey, White, Hood, & Sherif, 1961). Conversely, it is not only in the "real world" that situational factors and manipulations sometimes prove to be surprisingly small or nonexistent. It is the studies with detectable effects that get published, and the subset of the studies with large and unanticipated effects that become well known. The others languish in file drawers. We wish we had a dollar for every failed laboratory manipulation that social psychologists have designed with the confident expectation that the effects in question would be significant. What we have learned, in short, is that situational effects can sometimes be far different from what our intuitions, or theories, or even the existing psychological literature tell us they should be. Some factors that we expect to be very important prove to be trivial in their impact; and some factors that we expect to be weak prove, at least in some contexts, to exert a very large influence indeed. Accounting for our poor "calibration" as to the size of the effects produced by situational factors is a major focus of the education of the social psychologist and a chief concern of this book.

The Predictability of Human Behavior

When we, the authors, were undergraduates, we were assured that the sharply limited abilities of social scientists to make accurate predictions had to do with the relative youth of the social sciences. We no longer share such beliefs nor resort to such defenses of our field. We now believe that ours is not a particularly immature science and that we have, in fact, already discovered and documented some very important things about human social behavior. At the same time, we accept the fact that social psychology is never going to reach the point of predicting how any given individual (even one who is well known to us) is going to behave in a given novel situation. A corollary of this concession is that the application of social science knowledge is always going to be a risky business. When we try something new, even a new intervention that seems very reasonable on prior grounds, we are frequently going to discover that people respond quite differently than we had anticipated.

The roots of this fundamental unpredictability, we will argue, are very deep and perhaps akin to a source of similar unpredictability in phenomena in the physical and biological sciences (Gleick, 1987). We will consider this unpredictability issue further near the end of this chapter, and then return to it again at several other points throughout the book.

The Conflict Between the Lessons of Social Psychology and the Experience of Everyday Life

As we have seen, the evidence of empirical social psychology often conflicts sharply with what we "know" from everyday life. To be sure, we are sometimes surprised by the behavior of our fellow human beings, or by a genuinely unexpected act on the part of one of our children, or one of our friends, or some public figure. But for the most part the world seems an orderly, predictable place. It is extroverted Bill who dons the lamp shade at the party and not introverted Jill. Similarly, it is the pastor of the Church of the Good Shepherd who preaches charity and the Republican congressman from the wealthiest district in the state who preaches self-reliance and free enterprise. Moreover, soft answers do seem to turn away wrath. Sending a boy to do a man's job generally does result in disappointment. And, when it really counts, our best friends usually do come through for us, just as we had expected they would.

Earlier in their careers the authors seriously entertained the hypothesis that most of this seeming order was a kind of cognitive illusion. We believed that human beings are adept at seeing things as they believe them to be, at explaining away contradictions and, in particular, at perceiving people as more consistent than they really are. While we continue to believe that such biased processing of evidence plays an important role in perceptions of consistency, we now believe that the predictability of everyday life is, for the most part, real. At the same time, we believe that many of the principles and intuitions that people use to explain and predict behavior are unreliable. That is, people often make correct predictions on the basis of erroneous beliefs and defective prediction strategies.

We draw an analogy here between lay and professional physics. Lay physics (which is largely the same as Aristotelian and medieval physics) is undeniably mistaken in some of its main presumptions (Holland, Holyoak, Nisbett, & Thagard, 1986; McCloskey, 1983). In particular, lay physics, like lay psychology, errs in focusing on the properties of the object to the neglect of the field of forces in which that object ex-

ists. Moreover, the main interactional notion of lay physics—namely, the intuitive notion of "momentum"—is the utterly mistaken notion that a force applied to an object gives it a store of energy that gradually dissipates. The correct notion (that of inertia) requires that objects at rest remain at rest and that objects in motion remain in motion, unless some other force is applied. Nevertheless, lay physics does a perfectly good job of getting us through our days. In a world where air, land, and water all offer resistance or friction, the notion that objects some how lose their momentum is good enough. Only when we step outside the normal haunts of daily life, for example, when we venture into a physics laboratory or into outer space, does our lay physics get us into serious trouble.

And so it is for social psychology. Our intuitive ideas about people and the principles governing their responses to their environment are generally adequate for most purposes of the office and the home; but they are seriously deficient when we must understand, predict, or control behavior in contexts that lie outside our most customary experience—that is, when we take on new and different roles or responsibilities, encounter new cultures, analyze newly arisen social problems, or contemplate novel social interventions to address such problems. When we go from being students to being professionals, when we bargain with a street vendor 5,000 miles from home, or when our community begins a new program to deal with crack addiction or homelessness, the inadequacies of lay principles are likely to be revealed.

Much of what we wish to do in this book involves describing how lay social psychology differs from scientific social psychology. In this task we will identify three principles as the major cumulative insights of our field—a kind of tripod that provides the foundation for our collective enterprise. The first principle concerns the power and subtlety of situational influences. The second involves the importance of people's subjective interpretations of the situation. The third speaks to the necessity of understanding both individual psyches and social groups as tension systems or energy "fields" characterized by an equilibrium between impelling and restraining forces. We will sketch these principles here briefly and then illustrate their application throughout the rest of the book.

THE TRIPOD ON WHICH SOCIAL PSYCHOLOGY RESTS

The Principle of Situationism

Our discussion of situationism in social psychology must begin with an introduction to Kurt Lewin, a German emigrant who came to the United States in the mid-1930s. His contributions over the next decade

redefined the field of social psychology and continue even today to exert a profound effect on its major theoretical and applied traditions. Lewin's general theoretical formulation began with the familiar truism that behavior is a function of the person and the situation (or, in Lewin's terms, a function of the "life space," which includes both the individual and the individual's psychological representation of the environment). Despite the evenhandedness of Lewin's formulation, which cites the joint influence of situational and dispositional determinants of behavior, it was the power of the immediate social situation that was featured in his empirical work and that of his students. Lewin's particular concern was the capacity of situational factors, and social manipulations, to influence patterns of behavior that normally are seen as reflective of personal dispositions and preferences.

One provocative field experiment, for example, was conducted by Lewin, Lippit, and White (1939) at a time when the specter of Nazism was looming large for social scientists and for all humanity. The experiment featured a manipulation of leadership style to create authoritarian versus democratic group "climates" in recreation clubs (set up specifically by Lewin and company to conduct their study). This manipulation proved sufficiently potent to produce marked differences in the way that young male club members related to each other and to those with greater or lesser power. Scapegoating, submission to authority figures, and at times even expressions of hostility—in short, the disturbing complex of responses generally associated with the "authoritarian personality" (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950)—could be either inhibited or promoted, the Lewinians showed, by a relatively short-term manipulation of the person's immediate environment.

Even more important and illustrative of the tradition established by Lewin was a series of studies employing the then-novel technique of "group decision making" to facilitate changes in consumer behavior, health practices, and work-place productivity (for example, Bennett, 1955; Coch & French, 1948; Lewin, 1952). These studies, which we will describe in more detail in Chapter 8 on application, brought to bear a fundamental insight of Lewin's that is now familiar to a generation of organizational psychologists and "training group" practitioners: When trying to get people to change familiar ways of doing things, social pressures and constraints exerted by the informal peer group represent the most potent restraining force that must be overcome and, at the same time, the most powerful inducing force that can be exploited to achieve success.

Thus the main point of Lewin's situationism was that the social context creates potent forces producing or constraining behavior. He was well aware that these forces were often overlooked in lay psychology and that their identification was to be a major task of scientific social psychology. Indeed, Lewin explicitly noted the analogy we discussed

earlier between the errors of lay social psychology and the errors of lay physics.

An equally important part of Lewin's situationism was a healthy respect for apparently minor but actually important details of the situation. He often called these "channel factors" because they referred to small but critical facilitators or barriers. Lewin recognized that behavior often is produced by the opening up of some channel (for example, by public commitment to a course of action, or by taking a halting first step in the direction of some new behavior) and sometimes is blocked by the closing of some channel (for example, by failure to formulate a specific plan to carry out a concrete action at an opportune moment).

An example of how Lewin's channel factors work was provided by Leventhal, Singer, and Jones (1965). Their experiment dealt with the familiar problem of translating good intentions regarding personal health practices into concrete and effective action. Their subjects were all college seniors who received persuasive communications about the risks of tetanus and the value of inoculation. All subjects, furthermore, were told where they could go to get themselves inoculated. Paper-and-pencil questionnaires revealed that the communication was quite effective at changing the beliefs and attitudes reported by the students. Nevertheless, only about 3 percent actually took the step of getting their tetanus shots. By contrast, when subjects who had received the same communication also were given a map of the campus with the Health Building circled, and were urged to review their weekly schedule to decide on a particular time and route to get them to the Center, the percentage of takers went up to 28 percent. Clearly, learning the relevant information about the disease and its prevention, and even forming a general intention to take the necessary steps to protect themselves, were not enough for most subjects. It was also necessary, apparently, for them to have a specific plan (and perhaps even a map) for getting there—or, in Lewin's terms, a ready "channel" through which intentions could lead to actions.

Of course, 28 percent may still seem like a disappointing percentage for "medical compliance." We suspect that an even more specific invitation and channel—for example, an invitation to "show up next Tuesday at 10:00 A.M. when your schedule suggests that you will just be coming out of your chemistry class, with an hour to spare before your 11:00 A.M. Psychology 1 course,"—would have been even more effective in getting subjects on the path to the Health Center and to their tetanus inoculations. A similar point is made by many contemporary studies of the utilization of public health services. Attitudes and other "interesting" individual difference factors rarely do a very impressive job of predicting who will or who will not show up at the clinic or counseling facility. Instead, a more powerful predictor of usage is the mere distance of the individual from the closest service. Again, a simple

channel factor tends to override all others in predicting who uses the services (Van Dort & Moos, 1976).

The channel factor principle, thus, is one key to understanding why some situational factors have bigger effects than might be anticipated and why some have smaller effects. Seemingly big interventions and campaigns that provide no effective input channel in the form of situational pressures, or no effective behavioral outlet channel in the form of clear intentions or plans, will generally produce disappointingly small effects. And seemingly small situational factors that operate on important input or output channels will often exert gratifyingly large effects.

The Principle of Construal

The second enduring contribution of social psychology, ironically, is one that challenges the theoretical and practical value of the doctrine of situationism. The impact of any "objective" stimulus situation depends upon the personal and subjective meaning that the actor attaches to that situation. To predict the behavior of a given person successfully, we must be able to appreciate the actor's construal of the situation—that is, the manner in which the person understands the situation as a whole. Construal issues are similarly important if our goal is to control or change behavior. Many well-intentioned, even well-conceived, social interventions fail because of the way in which they are construed by the targeted group (for example, as an insulting and stigmatizing exercise in co-option or paternalism).

As we will spell out in Chapter 3, situationism in social psychology has similarities to the situationism of the behaviorist tradition. Both traditions were impatient with the lay (and psychoanalytic) emphasis on the importance of individual differences and unique personal histories, and both emphasized the importance of the immediately impinging stimulus situation. But the social psychological and behaviorist traditions parted company long ago over the issue of construal. The avowed goal of the behaviorists was to specify objective stimuli and the associations formed between such stimuli and observed responses without any attempt to look inside the "black box" of the subject's mind. Social psychology, however, as Robert Abelson has put it aptly to us in conversation, was the one field of psychology that could never really be "behaviorized." Its most astute practitioners always understood that it is the situation as construed by the subject that is the true stimulus. This meant that theory was always going to have to focus on subjective interpretations of stimuli and responses as much as on stimulus-response relationships themselves.

As early as the 1930s, European psychologists, such as Piaget and F. C. Bartlett, offered discussions of the importance of construal processes

and shaped research on the topic by introducing the notion of a "schema"—that is, a knowledge structure that summarizes generic knowledge and previous experience with respect to a given class of stimuli and events and, at the same time, gives meaning and guides anticipation with respect to similar stimuli and events in the future. Aside from Lewin himself, social psychology's most convincing advocate of the importance of paying attention to the actor's definition of the situation was Solomon Asch (1952). In Chapter 3 we will discuss the nature of Asch's subjectivist orientation, especially as he applied it in interpreting the results of his own research and that of his contemporaries.

More recently, social psychologists, together with their colleagues in cognitive psychology and artificial intelligence, have focused again on what might be called the "tools of construal." Discussion of cognitive structures (schemas, scripts, models, social representations) and strategies (judgmental "heuristics," tacit rules of conversation), and their role in helping people make sense of the events they observe, have become ever more frequent. We ourselves have labored hard in that tradition, and wrote a book in 1980 that was in large part an account of the layperson's tools of construal and of their shortcomings for the various tasks of human inference.

In this book we will once again devote attention to how construal influences behavior and how construal works. But our primary concern here is not documenting that subjective construal occurs or that it matters in determining how people will respond to their environment. What we seek to establish is that laypeople consistently fail to make sufficient allowance for the role that construal plays in determining behavior, a failure with profound personal and social consequences. In particular, we will argue that people make three distinct but related errors about construal.

The first error is a failure to recognize the degree to which one's own understanding of stimuli is the result of an active, constructive process, rather than a passive reception and registering of some external reality. There is an old joke about the three baseball umpires who were discussing their work. The first says, "I call 'em as I see 'em." The second says, "I call 'em as they are." The third says, "They ain't nothin' till I call 'em." Our contention is that, like the second umpire, most people are philosophical realists, with little appreciation of the extent to which their own cognitive processes have contributed to their judgments. Insight into the interpretive nature of judgment such as that shown by the first umpire is rare, let alone the extreme subjectivism of the third umpire.

The second error is the failure to appreciate the inherent variability of situational construal. The way any two people interpret a given situation, or even the way a particular person interprets identical stimuli on two different occasions, is only imperfectly predictable and is always uncertain to some degree. Because people fail to recognize the extent to which others may construe situations differently from the way they themselves construe them, they tend to be overly confident in predicting other people's behavior. They may even be too confident in predicting their own future behavior when the context for that behavior is novel or ambiguous. We argue that people make behavioral predictions with a degree of certainty that would be warranted if, but only if, their construals were both perfectly accurate and perfectly shared by the actor in question at the moment that the actor behaved.

The third error concerns causal attributions for behavior. People fail to recognize the extent to which observed actions and outcomes, especially surprising or atypical ones, may prove to be diagnostic not of the actor's unique personal dispositions but rather of the objective situational factors facing the actor and of the actor's subjective construals of those factors. In effect, people are too quick to "recompute" the person (that is, to infer that he or she is somehow different from other ordinary people) and too slow to recompute or reconstrue the situation (that is, to infer that one's original construal of the situation was incomplete, or erroneous, or at least significantly different from that of the actor). Finding that Jane the librarian has cast away job and home for an opportunity with a travel agency in a distant city, we are too likely to assume that Jane is a far more adventuresome soul than we had assumed and too little inclined to assume that the new employment opportunity is much more interesting (or that additional but hidden constraints on Jane were more weighty) than we had recognized. Much of our own recent research has been concerned with documenting these three errors and pursuing their implications. This research is presented in Chapter 3 on construal and in Chapter 5 on lay personality theory.

The Concept of Tension Systems

Social psychology's third major contribution, and the remaining leg of the conceptual tripod upon which our field rests, is the principle that individual psyches, as well as collectivities ranging from the informal social group to the nation, must be understood as systems in a state of tension. The analysis of any given stimulus situation must include the recognition first that "behavior has to be derived from a totality of coexisting facts," and second that "these coexisting facts have the character of a dynamic field insofar as the state of any part of this field depends on every other part of the field" (Lewin, 1951, p. 25). No simple mechanistic laws relating particular stimuli to particular responses are possible, given that both are always embedded in dynamic contexts that alter and constrain their effects.

> . . . such phenomena as the speed of production in a factory are the result of a multitude of forces. Some forces support each other, some oppose each other. Some are driving forces, others restraining forces. Like the velocity

> of a river, the actual conduct of a group depends upon the level (for instance, the speed of production) at which these conflicting forces reach a state of equilibrium. To speak of a certain culture pattern... implies that the constellation of these forces remains the same for a period or at least that they find their state of equilibrium at a constant level during that period. (Lewin, 1951, p. 173)

There are three major contributions of the tension system notion. The first is that an analysis of restraining factors can be as important to understanding and anticipating the effects of a newly introduced stimulus as an analysis of the stimulus itself. The effect of introducing a new monetary incentive for production in a factory depends on the balance of forces maintaining production at the current level. If there is a group norm against overproduction or "rate-busting," the incentive may have little effect or even a reverse effect. The dynamic contest between opposing forces was nicely captured by Wolfgang Koehler's concept of the "quasi-stationary equilibrium." This concept implied that certain processes or levels, like the velocity of Lewin's river or the level of production in a factory, fluctuate within the confines imposed by certain constraining and impelling forces. The level can be easy to move up or down within certain relatively narrow limits, harder to move beyond those limits, and virtually impossible beyond still further limits. Furthermore, change in the system can be accomplished in two very different ways with rather different consequences. One can add or increase impelling forces (and thereby increase the tension in the system as the relevant restraining forces increasingly make their opposing influence felt) or one can eliminate or weaken the restraining forces that impede the desired change (and in so doing decrease the tension in the system). For example, it may be more effective to change group norms about rate-busting than to promise ever-higher incentives.

The second important point is the converse of the first. Systems sometimes stand balanced precariously on the cusp of change. We may return to the river analogy by noting some interesting facts about the Mississippi. Basically, the river meanders through its last several hundred miles before spilling into the Gulf of Mexico in a general course that could not be altered by any event of less than cataclysmic proportions. But its local course is subject to drastic alteration by remarkably trivial events. A person with a shovel can, at the right place, start a small cut that gets bigger and bigger until the whole river flows through the new channel and an entire curve of the river is obliterated. (This fact was an ever-present consideration to nineteenth-century owners of river-front property, who often hired men to shoot on sight any suspicious persons caught upriver in the possession of digging implements.)

The analogy between the flow of a river and both individual and social psychological processes should be clear. Quasi-stationary equilibria can be hard to change because of the balance of opposing forces that maintain, and in a sense overdetermine, the status quo. On the other hand, very dramatic and widespread changes in the system can sometimes result from the introduction or alteration of seemingly small and inconsequential forces. Thus, the third point arising from the notion of a tension system results from the linking together of the first two points. Like the principle of construal, the tension system principle helps us understand why apparently big situational manipulations sometimes have small effects and why apparently small situational manipulations sometimes have big effects. Big manipulations may fly in the face of, or even increase the strength and resistance of, even bigger restraining factors. Conversely, small manipulations may take advantage of the precarious balance of the system, or facilitate an important channel factor, moving the system by redirection rather than by brute force.

We may illustrate these notions by reference to the astonishing events in the East Bloc countries that are unfolding as we write this book. For the 40-year period from the end of World War II to roughly 1985, the level of most internal processes within these countries, as well as the level of most of their external relations, was held within limits that now seem to us to be rather narrow. For a time there would be extreme repression of dissent, followed by slight letups in repression; for a time there would be some toleration of entrepreneurial activities, and for a time almost none. Thaws and freezes in the Cold War relations of these nations with the West took place within a range that we now recognize as fractions of a degree centigrade. Such slight movements up and down of social processes are well understood as resulting from a state of quasi-stationary equilibrium. Impelling forces were being met by restraining forces of equal strength. Changes of level of various processes were correspondingly held to small magnitudes.

As events of recent years have shown, however, these systems, though in equilibrium, were at very high levels of tension indeed. Both impelling and restraining forces were at massive strength. Correspondingly, when channels were opened up, change occurred at breathtaking speed, and it is already clear that the world landscape will shortly be unrecognizable to those born in the first eight decades of this century.

These events also make a humbling point about predictability in a world composed of tension systems. If anyone in the West had predicted, say in 1984, that the political and economic systems of the Soviet Union might soon be transformed by a liberalizing revolution from the top, followed very shortly by the end of Party rule in virtually all the East Bloc nations, that person would have risked being labeled a fool or a dreamer. It was obvious to all sensible analysts that the East Bloc countries could be expected to move only glacially toward change. Indeed, the past four decades should have been adequate evidence for that point for anyone so naive as to doubt it.

The social psychologist who applied the tension system notions to the most impressive effect was Leon Festinger. Festinger (1954; Festinger, Schachter, & Back, 1950) recognized that individual human attitudes are best understood as existing in a state of tension in relation to the attitudes of members of the face-to-face groups to which each person belongs. People do not like the state of being in disagreement with their fellows, and when they discover that this is the case, three balance-restorative processes are instituted—attempts to change others' opinions so as to move them into line with one's own, receptivity to others' similar attempts to change one's attitude, and a tendency to reject others from the group to the extent that they refuse to move toward the central tendency of opinion in the group. Festinger derived many interesting social phenomena from the operation of these processes, which we will begin to discuss in the next chapter.

Festinger also regarded attitudes within the head of the individual as existing in a state of tension. Some attitudes support each other; some contradict each other. Contradictory attitudes exist in a state of tension, called "dissonance," which must be resolved. One attitude or the other must be changed until the system is restored to a state of balance (see Festinger, 1957; Aronson, 1969).

Festinger's most dramatic use of the tension system notion was for cases in which the two cognitive elements in conflict are an attitude and a behavior. This occurs when someone does something that follows neither from the attitudes the person holds nor from some extrinsic force such as the expectation of reward. Festinger showed that in such a situation people can be expected to move their beliefs into line with their behavior. Thus if someone is maneuvered into delivering a speech that happens not to reflect the person's prior beliefs, and if the person is paid little or nothing for doing so, the person's expressed attitudes move in the direction of the position taken in the speech. This movement is blocked if the person is paid a substantial amount for delivering the speech. In this case, giving the speech is highly consistent with the payment and the person recognizes the lack of relation between prior beliefs and what was said.

The dissonance theorists' analysis of dissonance and attitude change spearheaded what had perhaps been social psychology's most important contribution to the study of motivation, that is, exploring the significance of perceived personal responsibility and choice (see Aronson, 1969; Calder, Ross, & Insko, 1973; de Charms, 1968; Linder, Cooper, & Jones, 1967). Social processes unfold quite differently when people believe they have freely chosen their behavior, as a direct expression of their goals and attitudes, than when they believe the behavior was coerced or was under the control of extrinsic reinforcing agents. People who are paid to deliver a speech think of their behavior as unrelated to their beliefs and their beliefs remain unchanged; people not paid to give the speech presume that it was

freely chosen and consequently feel compelled to realign their beliefs with their behavior. Factory workers ordered to carry out certain tasks in a certain order often function as inefficient automatons and sullen time-servers; the same workers asked to help design their jobs function as free agents with a stake in the success of the joint enterprise.

We will not give the tension system notion a chapter of its own, but we will refer repeatedly to it as we discuss the power of situations in Chapter 2, as we try to explain the bases of predictability of the social world in Chapter 6, as we explore culture and personality and try to understand the conditions of cultural change in Chapter 7, and as we analyze the fate of successful and unsuccessful social interventions in Chapter 8.

PREDICTABILITY AND INDETERMINACY

All three of the fundamental principles of social psychology speak in the most direct way to the question of prediction, both the ultimate predictability attainable by scientists and the typical level of predictability attained by laypeople in everyday life. We will be centrally concerned in this book with the ways in which the scientist and the layperson go about predicting behavior, what the limits of prediction may be, and how prediction can be improved. Let us anticipate our discussions of the two types of prediction we are concerned with.

Prediction by Social Scientists

We think that social scientists have been pursuing unrealistic goals of prediction. We may never be able to predict how particular people will respond to novel situations (either on the basis of personality assessments or on the basis of objective accounts of the situation). We also may never be able to predict how people in general or particular groups will respond to novelty. Situations are highly complex, and so are people's interpretations of them. One practical implication of this difficulty (discussed in more detail in Chapter 8) is that social remedies normally should first be tried out on a small scale. This applies even when the remedy in question has proved successful in some seemingly similar context. The surrounding matrix of situational forces and constraints may be subtly different, and the way people construe them may also be different, and the difference may be unanticipated by those planning and conducting the intervention.

We are neither apologetic about these limits to prediction nor distressed by their practical implications. They do not mean that we cannot effectively intervene to better the lives of individuals, groups, or so-

ciety as a whole. The constraints merely indicate that there are limits to what is possible, and it may take some tinkering, using the best hypotheses of our science and the results of some careful pilot testing, before we can achieve those possibilities.

The other reason we are unapologetic is that the situation in the social sciences is not fundamentally different from the situation in the physical sciences. It has long been recognized that the laws of physics do not allow us to predict with much certainty where any particular leaf from a tree will fall. More recently, physical scientists have begun to recognize the limits of predictability in a variety of systems, such as ecological systems and weather systems. Although some effects are robust and highly predictable, others are extremely unstable. The term "butterfly effect" has been coined to describe small, unanticipatable perturbations that can have dramatic effects (Gleick, 1987). The whimsical name refers to a meteorologist's comment that a butterfly beating its wings in Beijing can, under the right circumstances, have a detectable effect on the weather in the midwestern United States a few days later. As a consequence of the extreme sensitivity of weather to local perturbations, long-range weather forecasting not only is not possible now but also, according to some scientists, will never be possible. A similar point can be made for ecologies. Sometimes the introduction of larva-eating beetles has just the desired effect of consuming all the targeted noxious insects. Sometimes the creatures introduced are immediately eaten by a predator and vanish. Sometimes the creatures become themselves a greater scourge than the one they were intended to replace.

Again, there is a real question as to whether such effects can ever be predicted with precision in highly complex, interactive, nonlinear systems. But the discovery and description of the sources of such inherent unpredictability, whether in the physical sciences or the behavioral sciences, is hardly a cause for apology. It is an important intellectual contribution with profound theoretical and practical implications.

Prediction by Laypeople

We are even more interested in the implications of social psychology's basic tenets for the layperson's predictions than for those of social scientists. We wish to demonstrate that, for reasons that make sense in terms of the three major principles we have outlined, lay predictions are often both wrong and too confidently made. To begin with, people are apt to have exaggerated notions about the strength of individual differences and the role such differences play in producing behavior. Some of the reasons for this are essentially perceptual. The continuity in Ralph's physical appearance and personal style (for example, his imposing stature, deep voice, steady gaze, and habit of clenching his fist

to emphasize his words) may blind us to the lack of any real consistency in the degree of dependency or aggressiveness he shows across different situations. Other factors are more cognitive. Inconsistent data typically are assimilated in a way that produces illusions of past behavioral consistency. Our first impression that Ellen is friendly leads us to interpret her sarcastic response to Bill's whispered remark as jocular, or a justifiable reaction to what Bill must have said, or perhaps the result of pressures she has been under at work; but not as evidence that our earlier impression was wrong, and that Ellen is simply variable in her friendliness.

Beyond discussing such sources of illusory consistency, we will emphasize the extent to which uncertainty in the way particular individuals construe particular situations, and the difficulty in predicting such construals, necessarily limit the amount of observable cross-situational consistency that ever *could* be demonstrated. Ellen's friendliness, or lack of it, in particular social situations will depend on the way she labels those situations and resolves any ambiguity about the meaning of any behavior directed toward her.

At the same time, we will contend that people do, in fact, manifest considerable predictability of a sort that observers can perceive and make use of in their everyday social dealings. The apparent conflict between the lessons of formal research and the lessons of everyday experience, we believe, results from the investigator's reliance upon research strategies designed to disentangle the separate contributions of person and situation by exposing some sample of individuals to a fixed, and identical, set of situations. This strategy, despite several undeniable advantages for the theoretician, can lead us to ignore some important realities about everyday life. Foremost is the fact that in everyday experience the characteristics of actors and those of the situations they face are typically confounded—in ways that contribute to precisely the consistency that we perceive and count on in our social dealings. People often choose the situations to which they are exposed; and people often are chosen for situations on the basis of their manifest or presumed abilities and dispositions. Thus, clerics and criminals rarely face an identical or equivalent set of situational challenges. Rather, they place themselves, and are placed by others, in situations that differ precisely in ways that induce clergy to look, act, feel, and think rather consistently like clergy and that induce criminals to look, act, feel, and think like criminals.

We also will explore the implications of the fact that people sometimes feel *obliged,* even committed, to act consistently. This may be because of their social roles, because of the real-world incentives and sanctions that await those who honor or violate such roles, because of promises they make to others or even because of demands they place upon themselves. The net result of these influences is that we correctly

anticipate a predictable social world, one with consistent, or at least coherent, actors. This result, moreover, is especially likely to be true in the domains that we care about most and in which we have the most experience.

Finally, it should be noted that both consistencies and seeming inconsistencies in behavior can sometimes be reflections of individual differences in the construal processes that people bring to their understanding of their social environments. Here we follow a strain in personality theory that has its origins in Freud, was developed by George Kelly (1955), and finds its modern fruition in work by Mischel (1973), Markus (1977; Markus, Smith, & Moreland, 1985), and Cantor & Kihlstrom (1987). Each of these theorists has contended that the key to a more powerful conception of individual differences is to be found in the enduring motivational concerns and cognitive schemes that guide attention, interpretation, and the formulation of goals and plans. An important consequence of this contention is that behavioral consistencies, where they are found, may not be well captured by traditional personality traits. That is, individuals may behave in consistent ways that distinguish them from their peers not because of their enduring predispositions to be friendly, dependent, aggressive, or the like, but rather because they are pursuing consistent goals using consistent strategies, in light of consistent ways of interpreting their social world (cf. Cantor & Kihlstrom, 1987).

In short, our overall thesis, developed in detail in Chapter 6, is that some of the layperson's most fundamental assumptions about personal consistency and predictability are validated by everyday experience, even though the basis for such consistency may be misunderstood by the perceiver. Thus, despite the demonstrable errors and biases of lay prediction, the world as it is experienced daily is, in fact, a reasonably predictable place. Lay psychology, like lay physics, generally gets the job done reasonably well using dramatically mistaken principles; and when it fails, it will generally be for reasons that rather deep principles of our discipline allow us to understand and sometimes even anticipate.

THE PROBLEM OF EFFECT SIZE

Implicit in our discussion to this point is that some effects are clearly big and some are clearly small, that some levels of predictability are demonstrably high and some are demonstrably low.

Consider our claim that demonstrating the power of the social situation has been one of social psychology's most important contributions, and that failing to demonstrate the power of classic personality

traits or dispositional differences between individuals has been one of personality psychology's greatest frustrations. Implicit in such a claim is the suggestion that the relevant situation effects are in some obvious sense "big," and that the relevant person effects are in some obvious sense "small." It will be useful to offer some initial thoughts on the question of how to measure, or even to think more clearly about, effect size. This question turns out to be surprisingly controversial and difficult to answer; but we will do our best to begin shedding some light on it, because it is so fundamental to the concerns of this book.

Let us begin by noting that effects are big, or small, relative to something. For our purposes it will be sufficient to refer to three definitions of relative effect size, which we will call the statistical, the pragmatic, and the expectational.

Statistical Criteria of Size

In considering statistical criteria, we must begin by noting that effect size has very little to do with statistical significance. An effect of almost any size can be made to be statistically significant (that is, unlikely to have occurred solely by chance) merely by collecting a large enough number of observations. One of the authors had this point brought home with particular force in graduate school, when he opened the computer printout of analyses of a national survey, ran his finger down the column to the relationship he was particularly interested in, noted that the correlation was statistically significant at the conventionally accepted .05 level, and jumped for joy. His companion was required to note that the correlation the author was getting so excited about was .04—a degree of relationship very close to zero. This trivial correlation was significant because the survey had well over a thousand respondents. Thus the author was right about this prediction—the relationship he proposed was there—but it was so weak that it could have no theoretical or practical significance.

A much more sensible convention for defining effect size was suggested by Cohen (1965, 1977), who suggested that the magnitude of experimental effects should be judged relative to the variability of the measure in question. Thus, by Cohen's criterion, a difference between two means that corresponded to a quarter of a standard deviation in the distribution of the relevant measure would be deemed small, a difference corresponding to half a standard deviation would be deemed moderate, and a difference corresponding to a whole standard deviation would be deemed large. This statistical definition, and other related ones, assess effect size relative to all nonspecified, "random" determinants of variability or, in other words, relative to "noise." The defi-

nition effectively finesses, in fact ignores, all considerations of the nature of the variable under consideration and the units of measurement involved. Therein lies both its major virtue and, as our discussion of the other two criteria will make clear, also its chief drawbacks.

Pragmatic Criteria of Size

The most telling objection to a simple statistical definition hinging on standard deviations is that in many cases we don't care in the slightest about some effects that would qualify as "big" by this definition or, conversely, that we care a great deal about other effects that would be termed "small" by this definition. Imagine, for example, that you are told that some exotic new drug can increase the survival time of people stricken with Smedley's Fever by 1.5 standard deviations. Interested at first, you then find out that Smedley's Fever is a virulent tropical disease to which untreated sufferers succumb after 40 hours on average, with a standard deviation of 4 hours. This means that the drug could prolong life for, on average, an additional 6 hours. If you next find out that the drug costs $10,000 per dose, your already diminished interest approaches the vanishing point. (On the other hand, the medical researcher who seeks to unravel the mysteries of this illness or related ones might jump at this clinically trivial improvement because it could hold clues that might lead to insights and advances that really would be big.)

Conversely, imagine the plight of a political candidate involved in a close contest. The candidate may be quite willing to spend a monumental sum on an advertisement or a campaign strategy that would influence the proportion of the total votes he or she would receive by less than one-tenth of a standard deviation [e.g., .05 of the votes cast, by the conventional formula whereby the standard deviation of a proportion (p) is equal to the square root of $p(1 - p)$ or, in other words, the square root of $.5 \times .5$]. Most political pundits would agree that the effect of any advertisement or strategy that could produce a "5-point" swing in the vote should be termed "big." (It would have been big enough, notably, to change the results of roughly half of the American presidential elections in the twentieth century.) Similarly, as we will discuss in more detail in Chapter 4, a personality test that was inexpensive to administer and that could predict "only" 10 percent of the variance in some important outcome, could prove to be very valuable and "cost-effective" for many familiar assessment or prediction tasks, for example, for selecting people who are likely to be extreme on some dimension (see Abelson, 1985).

These examples demonstrate that utilitarian considerations almost inevitably influence our judgment about whether an effect is big or not. Effects are big or small relative to the obstacles that stand in the way of getting a particular job done, and relative to the importance of the job—that is, big or small in terms of their sufficiency for accomplishing specific objectives, and with reference to how much we care about those objectives.

Expectation Criteria of Size

Finally, and perhaps most important for our purposes, effects may be regarded as big or small relative to what we expected them to be. We may call this the expectation criterion because it involves changes in one's previous beliefs (or "Bayesian prior") with respect to some outcome or event. By this criterion, effects are big if the relevant data force big revisions in our expectations and in the theories that govern those expectations and effects are small if they force little or no change. It is worth noting in this context that very small effects (small, that is, by conventional statistical standards) can sometimes force us to rethink very basic and well-established theories—provided that we had a very well-grounded basis for expecting no difference at all, and provided that we had a very precise measurement technique for establishing whether there really was or was not any difference found.

Outcomes thus can be assessed in terms of their capacity to alter our subjective probabilities. When Senator Snort, who was expected to run fifth in the New Hampshire primary, manages to run second instead, we feel that he won a "big" fraction of the vote. When Governor Grump, expected to win the primary, comes in second instead, we may feel he won a "small" fraction of the vote. In both cases, we label the campaigns they conducted as "successes" or "failures" as a function of their effectiveness relative to our prior predictions and beliefs.

The judgments passed on social interventions and on the scientific theories on which they are based depend on how well they do relative to our expectations. Even a well-established theory may become ripe for re-examination when predictive chinks in its armor are discovered, and theories that are very implausible on their face gain substantial credibility when their progenitors make a prediction or two that are contrary to the received opinions of scientists but turn out to be correct. This final definition has an interesting and important consequence. Any experience, training, or even rhetoric that influences our expectations thereby influences both our assessments of the size of any given effect and our satisfaction or disappointment with the interventions that produced that effect. The positive effects of social interventions such as Operation Headstart (the preschool educational intervention program for disadvantaged children) and racial integration of schools are real enough, though not always statistically large. But in terms of the political and

social science rhetoric of the era in which these interventions were introduced, and the resulting great expectations, the effects were widely dismissed as trivial and as grounds for de-emphasizing rather than maintaining and strengthening such programs in the future.

It should be noted that in this book when we speak of big situational effects, we normally will mean that the effects are big by at least two of these standards—statistical and expectational—and sometimes by the pragmatic criterion as well. When we speak of small dispositional effects, we normally mean that the effects are small by the same two standards—statistical and expectational—and usually by the pragmatic criterion also. When we speak of the effects of interventions and applications, we normally measure size by the pragmatic criterion alone.

When we compare effects, we will present results wherever possible in proportional form. Thus, in reporting the effects of experiments or interventions, we will report the proportion in the experimental condition and in the control condition who behaved in a particular fashion or who had a particular outcome. In reporting the differences associated with personality traits, we will compare the proportion above and below the median or at two standard deviations above the median versus two standard deviations below the median, who behave in a particular fashion. The proportional measure of effect size is, of course, associated with each of the three criteria of effect size, but only in a rough and highly variable way. Its great virtue is that it is a common metric readily understandable by everyone. Partly for this reason, it is the most efficient metric to use for estimating effect size by the expectation criterion. As we will see in Chapter 5, it is easy for people to convert their expectations about effect size to estimates of proportions, and then to compare these with actual proportions.

OVERVIEW AND PLAN OF THE BOOK

In summary, this is a book about the predictability and coherence of behavior as seen from the perspective of modern experimental and cognitive social psychology. We begin with the history of research suggesting that situational factors often prove to be more powerful determinants of behavior than the vast majority of us—scientists and laypeople alike—would have guessed. Implicit in this situationist lesson is the suggestion that people from different backgrounds, people with different beliefs, even people with apparently different personalities, must understand and react to some situations rather uniformly. In other words, there are at least some important respects in which human beings prove to be more alike than we generally reckon them to be.

At the same time, research and everyday observation constantly remind us that people often do differ dramatically both in their responses

to particular situations and events and in the patterns of their everyday behavior. We will argue that the shared convictions that laypeople have about stable, consistent, coherent, and predictable individual differences are not always mere cognitive illusions. Rather, they are based, at least to a substantial degree, on the data of everyday experience. Far from disputing the existence or significance of individual differences, we will acknowledge them and then explore their bases and implications. More specifically, we will provide a "situationist" and "subjectivist" account of individual differences—one that gives heavy weight to the complex dynamics of social systems and to the role of construal processes. Our goal will thus be an account of individual differences that seeks to explain what kinds of differences are likely to exist and be important, when they are likely to be obscured, and when misinterpretations of such differences are likely to arise.

Beginning in Chapter 2 we will illustrate what we mean by the power of situational factors by reviewing some of the classic studies of social psychology. In that chapter we will focus first on group influences and then on the notion of channel factors as conduits and barriers that facilitate or inhibit behavior change. In Chapter 3 we will discuss the significance of construal processes. There we will reiterate the truism that construals vary among individuals and are significant determinants of social behavior. More importantly, we will stress the fact that people may characteristically fail to recognize and make allowance for the vagaries of construal, both in predicting their own behavior and in predicting and interpreting the behavior of others. The consequence of this failure is that people too frequently make wrong predictions about behavior and then compound their errors by explaining the discrepancy from expectations in terms of stable dispositions of the actor.

The next four chapters of the book deal explicitly with predictability of individual behavior. We begin in Chapter 4 by reviewing some major studies documenting the modest size of the cross-situational consistencies in the behavior of people exposed to a fixed set of situations—in particular, the consistencies in behavior seemingly relevant to classic personality traits like extroversion or honesty. We will then show, in Chapter 5, that these data are indeed surprising to people, that is, that lay beliefs in consistency and predictability are mistaken, both qualitatively and quantitatively, in ways that no refinements of measurement or definition can remedy. In Chapter 6 we will discuss what we believe to be the sources of real behavioral consistency and predictability, some of which involve individual differences in roles and other situational demands, and some of which do not involve stable individual differences at all. In Chapter 7 we will turn our attention to old but lately neglected questions of cultural effects on behavior, again highlighting the role of situations, construal, and tension systems. We will argue that different cultures, including identifiable local subcultures within modern Western societies, effectively place actors in different situations, expose

them to different social dynamics, and lead them to habitual differences in construal that have real consequences for social actions.

In our eighth and final chapter we will speculate about the implications of the analyses of the preceding seven chapters for questions of intervention and social change. We will discuss some applied research that we think illustrates the value of the situationist, subjectivist, and tension system traditions discussed throughout this book. Our analysis seeks to explain why some kinds of interventions that one might expect to be powerful generally yield disappointing results, and why other, seemingly less powerful (and less expensive) ones may yield better results. This analysis helps illustrate the lessons that applied practitioners can learn from the best traditions of theoretically oriented social psychology, and the lessons that theoreticians can learn from the history of successful and unsuccessful applications. We believe it also offers important lessons for the layperson who attempts to apply social psychology in the conduct of everyday life, and in contemplating society's attempts to grapple with its most pressing social problems and challenges.